Rearing of chinook salmon fry in the Sacramento River

Larry J Hansen

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0217: Rearing of chinook salmon fry in the Sacramento River

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Guiding hypothesis of this proposal is that preferred juvenile Chinook habitats can be identified by patterns of residency and higher growth rates. PI s propose to sample three classes of juvenile Chinook salmon rearing habitat types (good, medium and poor) within the Sacramento River and a 1st Order Stream. Habitat types will be classified in year−1 and then habitat classes will be sampled for small wild juvenile Chinook salmon. Shoal regions of the Sacramento River (< 2 m) will be classified as good, medium, and poor according to attributes related to levee setback, silt levels, and vegetation based upon review of available GPS databases. Individuals will be tagged with CWT tags and some will be sampled for otoliths. From these directed sampling efforts residence time and growth rates will be estimated and serve to index rearing habitat value for wild Chinook salmon.

Additional Comments:

Strength of the proposal is the focus on small (

Guiding hypothesis of this proposal is that preferred juvenile Chinook habitats can be identified by patterns of residency and higher growth rates. PI s propose to sample three classes

of juvenile Chinook salmon rearing habitat types (good, medium and poor) within the Sacramento River and a 1st Order Stream. Habitat types will be classified in year−1 and then habitat classes will be sampled for small wild juvenile Chinook salmon. Shoal regions of the Sacramento River (< 2 m) will be classified as good, medium, and poor according to attributes related to levee setback, silt levels, and vegetation based upon review of available GPS databases. Individuals will be tagged with CWT tags and some will be sampled for otoliths. From these directed sampling efforts residence time and growth rates will be estimated and serve to index rearing habitat value for wild Chinook salmon.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Rearing of Chinook salmon fry in the Sacramento River

Reviewers did a thorough job, but reached different conclusions. Panel recognized general overall problems with how data will be treated, particularly with statistical analysis. There was a glaring omission in not thinking about temperature. The proposal design was not convincing in terms of how investigators can identify preferred habitat. The proposal has a very complex design with replication, but panel recommended that it would be important to include mark−recapture analysis. The proposal was unduly short.

Final Ranking: Adequate

Technical Review #1

proposal title: Rearing of chinook salmon fry in the Sacramento River

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**This proposal clearly states that the objectives of this study are twofold: 1) to determine if chinook salmon temporal residency patterns for preferred rearing habitat can be determined, and 2) to measure chinook salmon growth rates associated with these "preferred rearing habitats". The proposal authors, Hansen et al., hypothesize that preferred habitats do exist and feel that the metrics being tested by the two study objectives (site fidelity and growth, respectively) will adequately address the rearing value of these shallow water habitats.** **The objectives of this project remain internally consistent throughout the proposal. Although, the objectives only apply to the second and third year of the project because the first year will be spent selecting study sites. I think this study would benefit if the investigators had pre-chosen their sampling sites and had scheduled to do a pilot study during the first year to verify their site selection. This would enable Hansen et al. to use adaptive management to plan for subsequent field seasons after determining potential pitfalls (insufficient fish numbers) and unforeseen complications (changing river conditions) to ensure success in the second and third years.** **The idea of identifying "preferred rearing habitats" is important in the context of obtaining a better**

	understanding of the early life history of Central Valley chinook salmon and maintaining a healthy, viable population. Even more importantly is the fact that the focus of this study is on wild juvenile salmon. Much of what is presently known about the Central Valley fishery is based on hatchery fish or results drawn on sampling a mixed population of both wild and hatchery fish. In a time when there is a large degree of uncertainty and skepticism on whether to classify hatchery and wild fish as one and the same, or as separate stocks, we need to carefully design studies that will help answer this long−standing debate. **The implications of this project are not only important, but they are timely as well. Hatchery juvenile production and adult spawner returns are current measures being used to gauge the success of hatchery operations in sustaining certain fish stocks. If studies like this one can identify habitats with high restoration value that could enhance wild fish populations without increasing hatchery production, then the overall watershed may benefit by restoring degraded habitat in lieu of just adding more artificially reared fish. In addition to the wild versus hatchery debate, there is the ever−present and timely concern regarding California water allocation. If seasonal shallow water habitats are indeed important for sustaining wild fish stocks during certain times of the year, then this research may improve water managers ability to minimize the effects of flow regulation on important rearing habitats during critical parts of the lifecycle.**
Rating	**good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**This study is testing a justifiable and pertinent question that should provide a better understanding of the relative importance of wild chinook salmon freshwater habitat use. Hansen et al. have designed their study after Connor et al. 2003, who examined a similar question studying chinook salmon habitat use in the Columbia River. Connor et al found that wild chinook salmon residency did occur in certain portions of the Columbia River. Hansen et al. interpret the Columbia River findings as indicating that chinook salmon juveniles will reside in favorable habitats during their seaward migration and that resource managers may be able to apply this knowledge to restore Pacific salmon stocks and their ecosystems. Hansen et al. draw on their previous experience working in the Sacramento River system in order to demonstrate that a similar situation may exist here in the Central Valley to the Columbia River study. Previous Sacramento River studies indicate that chinook fry in the smaller size classes take longer to reach the river mouth (using tagged hatchery fish); and similar sized fry are captured at sites that are almost 100 river miles apart on the same sampling dates. It is unclear if small chinook salmon fry seek out "preferred rearing" sites where they maximize growth, but it is clear that you simply cannot use fork length as a metric to track movement of fish through the system. The assumption being made is that if better rearing habitats do indeed exist, fish will find them and spend a greater amount of time using them. A conceptual model outlining the expected responses to the proposed experimental design is included. The model is presented in a clear manner that further simplifies the underlying study hypothesis that fish will use/benefit the most from the highest quality habitats. The two metrics being measured (residence and growth) are presented in a simplified manner showing how the hypothesis will be tested. The part that is missing from the model is the justification for why shallow water river habitats (and 1st order tributaries) are the only habitats**

being investigated by this study. There is an increasing body of knowledge suggesting that floodplains, deltas, sloughs and estuaries all provide important shallow water habitats that have been shown to be valuable rearing habitat for pre-smolt salmon. If evidence exists for why only 'riverine' habitats are being analyzed then this information would provide strong justification to fund this study, but it is not specified. If this rationale does not exist, a more robust study design would include study sites from all 4 of these shallow water rearing habitats. Therefore, a comparison could be drawn b/w sites to measure the respective residence and growth rate for each habitat type.

Justification is lacking in this proposal to dedicate an entire year of funding to "Habitat Selection". This study appears to be carefully planned, and draws on historical data numerous times to justify the underlying rationale. Therefore, it is unclear why the study sites haven't been predetermined. In reviewing this proposal I found it would have been very helpful to have had a map of the entire study area, and the locations marked where the researchers intend to carry out their sampling. At the very least, they need to provide a detailed methodology of how they will use the data available to them (aerial photos, historical data, and GIS) in deciding on where to sample. Because sites were not selected before submission of the proposal, I see this as a major impediment in the review process since the entire study design is not predetermined. At a minimum, sites could have been selected base on sites that fit their categorical classifications for the first year of the study as a "Pilot phase". Modifications could then be implemented using adaptive management to select final sites for years 2 and 3. The chances are good that some sites would have remained the same throughout the entire study yielding a third year for data comparison. Furthermore, a percentage of those wild fish tagged in 2006 could potentially provide adult return data in

	the final year of the study (2008) which would enable the researchers to evaluate "preferred rearing habitat" use within the context of the entire life cycle using various scale and or otolith methodologies. This study is proposed as a full-scale 36 month project.
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The fish collection suggested in this proposal appears to be well planned. Using historical data, the research team should be able to select sites that will have sufficient fish numbers throughout the sampling period. The Catch Per Unit Effort data spanning the last ten years indicates that chinook fry are easily captured using a beach seine 70% of the time, averaging >30 fry/haul. As a result, obtaining a large enough fish sample size to ensure statistical significance should not present a problem in satisfying the growth component of the study. Otoliths are proposed to be used to satisfy numerous project objectives, including stock identification, back-calculations of fish length at time of tagging, and daily growth rate calculations. All proposed use of otoliths for this study are sound in methodology, but may require a greater amount of time to fulfill than Hansen et al. have accounted for. Since the proposal states that, "the left sagittal otolith will be removed from each recaptured fish", obtaining a larger number of otoliths than anticipated**

will cause difficulties in completing all the scheduled otolith work on time. If this project successfully recaptures their 30% target of the marked fall chinook salmon, then they will have 14,400 otoliths to process (16 units X 3 trials X 1000 CWTs). A modest 10% recapture rate will still yield 4,800 otoliths. See "Feasibility" section below for a breakdown on the laborious nature of fish otolith research and application.

In addition to otolith labor concerns, the "Personnel" section of the proposal does not indicate that any of the project personnel have any otolith experience. If any of the primary staff had prior experience reading chinook salmon otoliths, then this would be less of a concern as they could train and closely monitor their technicians that will be interpreting the otoliths. The proposal does not indicate this is the case. The experience level of the otolith reader(s) is a primary concern since the task was not specified to an experienced contractor, and no mention was made as to how the technician(s) will be trained (e.g. using a known-age otolith set, hatchery fish check identification, etc.). While reader accuracy error can be minimized by use of an otolith reference collection, precision error is commonly reduced (improved) by resolving interpretation differences among readers. This process is commonly called a "double-blind" study in which two experienced otolith readers read randomly coded samples, and their reads are compared. In the event of >10% disagreement between readers, samples are re-read and if the error is not resolved the samples are not included in any further analyses. This proposal indicates that one technician will be conducting the otolith

reads and plans to use a reference collection for quality control are not mentioned. Some fish are difficult to age and precision errors are always inherent at some level, therefore, Hansen et al. are exercising poor judgment in the interpretation of data that will be vital to the success of this project.

To measure fish residency in the study habitats, fish will be marked with CWTs and released back into their habitat unit. The recapture effort will be conducted randomly on the second through the fourteenth day post−tagging. This approach would be fine if the goal was simply to determine if any of the fish resided in shallow water habitats (yes/no response), but instead the goal of this project (as indicated in Figure 1 conceptual model) is to measure length of residence between the four basic habitat types. Additionally, Hansen et al. will be removing/killing fish for otolith growth analysis as quickly as 2 days after tagging. This proposed approach will only allow the researchers to document residence patterns up to 14 days, even if the fish would have resided for an extended period of time in the respective habitat unit.

An alternative approach for measuring fish residency using the proposed study design would be to sample on a weekly basis post−tagging to ensure a minimum of seven days of fish growth for otolith microstructure analysis. When the number of marked fish per seine haul falls below a threshold number, then the field crew would switch their sampling effort to tagging again to restock the habitat unit. This approach would enable the team to only remove a statistically significant number of

recaptures each week, presenting the remaining marked individuals that were not sacrificed for their otoliths with the opportunity to reside longer in the habitat. This way the maximum period of fish residence is not defined by the experimenter, but rather controlled by the environmental cues that are being tested in this study. A second alternative that would potentially provide better residence data would be to use passively integrated transponder tags (PIT) in lieu of the CWTs (similar to Conner et al. 2003). The numerous PIT advantages include: could record fish growth without sacrificing the fish, continuous collection of residence data, no need for an external mark. The disadvantage of using PIT tags is their cost, but fewer fish would need to be marked since tagged fish could be recaptured more than once. Speaking of external marks, Hansen et al. did not mention how the field crews will be able to differentiate between wild fish that they clipped the adipose fin from and those fish being released by the hatcheries. If there is no way to tell, then there is the potential for mistakenly killing a larger number of hatchery fish that are migrating through the study sites.

The potential results of this study are interesting, but are limited in their ability to benefit water resource management decisions. Because this study is designed to investigate the short term residence patterns of these fish, it will be difficult to extrapolate the significance of these findings in determining critical rearing habitats. Instead of trying to detect micro-residence patterns, research of this type should be focused on what habitat types do salmonids reside in for extended periods

	of time. Nearshore shallow water habitats in the Sacramento River may indeed be valuable habitats for juvenile chinook salmon, but the results of this research can only provide a limited insight into how important they really are in the overall freshwater lifecycle. An experimental design that measures how long individual fish remain in different habitat types encountered throughout their early life history would be more valuable to fishery and water resource managers. A technique that provides a comparison of survival to adulthood, also known as smolt−to−adult survival, that could separate fish that utilized different juvenile rearing strategies would be much more useful in answering the underlying hypothesis presented in this proposal. The guiding hypothesis is that extended residence and higher growth rates in select habitats will increase salmon survival. The major problems with the presented approach stem from the researchers picking these "preferred habitats", and the primary indicator to measure this preference metric is residence which is also defined arbitrarily by the research team (maximum of 14 days).
Rating	**fair**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**There are certain aspects of the proposed study design which are well documented (e.g. fish collection, tagging goals), while numerous uncertainties exist with other project objectives. Below is my review of the feasibility of the different tasks proposed, listed in order of Task ID number.**

I. Habitat Selection: Five months is an overestimation of the time to select study sites for this project. As indicated earlier, I believe this task should have been completed prior to the submitting this proposal. The executive summary states that, "previous sampling by the Stockton Fish and Wildlife Office since 1980 indicates that sufficient fry will be available". There is no justifiable reason why the previous 24 years of data couldn't have been summarized prior to submitting this proposal and used to determine suitable sampling reaches.

II. Field Trails: The proposed start for this task is month number 13, and ending month 29. This is one of the strengths of this proposal indicating that the Project Team has a good grasp on the feasibility of the fish collection and mark–recapture component of the study. The uncertainties regarding catching too many or too few fish are addressed in their sampling protocol, demonstrating the staff experience in planning this type of study design. An assessment of alternative marking methods was performed, and rationale was given for why Hansen et al. chose to use CWTs.

III. CWT Recovery &Reading: The proposed start for this task is month number 14, finishing up with all reads by month 30. Sixteen months seems adequate to read the goal number of 16,000 fry that the team anticipates recovering. But the feasibility becomes more problematic when we learn that the same biological technicians (n=2) that are conducting the field trials will also be performing the CWT decoding. It is important to note that the timeline for this task overlaps with the same 15 months the technicians will be conducting their field work, leaving only one entire uninterrupted month to read tags. It appears that this task will be understaffed and that successful completion will depend on how smoothly the field trials proceed and on the ability of the technicians to work efficiently in limited, small

	blocks of time between field excursions. **IV. Otolith Growth: The proposed timeline for this task is the same as task III, completing all otolith analyses by month 30. Due to the time intensive nature of otolith studies, I do not believe this task is feasible within the time allotted. Here again, the same technician that will be out in the field tagging fish up until month 29 will also be dissecting, cleaning, mounting, grinding, polishing, and interpreting the goal number of 4480 otoliths. Unlike task III which will be spilt between both technicians, this task is scheduled to be completed by just one technician. Using the rationale of Hansen et al. that an experienced reader can process 10 otoliths/day (which I strongly disagree with having processed ~ 1000 salmonid otoliths for my MS project), they will need a minimum of one technician devoting 448 days to this task. Add in training time with a reference collection and time spent comparing reads with a second reader and it becomes obvious that successful completion of this task will require hiring a full−time laboratory technician devoted entirely to otolith lab work.** **V. Database Management &Analysis: The proposed timeline for this task is 21 months in duration, starting one month prior to the field studies. This seems adequate to get the database template established and to ensure all analyses will be completed on time.** **VI. &VII. Reporting: The time allotted to prepare both the semiannual annual and the final reports appears to be feasible.**
Rating	**fair**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Interpretation of study findings may be difficult to interpret because the study design assumes that the "preferred rearing habitat" is encountered by the time fall juvenile chinook salmon have reached River Mile 80. It is quite possible that there is "critical rearing habitat" that will not be sampled by this study because it is inaccessible or may simply be overlooked in the design of the study. Another concern is that because this study is focused on "Sacramento River shallow water habitat", delta and estuarine habitat types are not even being included as possible preferred rearing habitats. I would like to see a more inclusive approach to investigate this question of residency patterns and growth rate performance during the chinook salmon freshwater life−history. An alternative study design combining otolith chemistry and otolith microstructure would provide more conclusive evidence on what habitat types are most important in the juvenile lifecycle prior to ocean entry. Using this combination of otolith techniques, comparisons could be made between fish that migrated slowly through the riverine environment, those that chose to reside in delta habitats instead of the mainstem river, and those that rapidly migrated through the riverine habitat opting to rear in a**

	brackish/estuarine environment.
Rating	**fair**

Additional Comments

Comments	**No study site map was included to help orient the reviewer with the study system and experimental design. The only mention of the proposed study design is that it will occur between river miles 80-220. These abstract boundaries are uninformative to this reviewer who does not work on the Sacramento River.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The Primary Staff is composed of a competent team of scientists that has extensive large-project management experience. The proposal is carefully written in regards to the area of specialization of the authors. Both Hansen and Bellmer have experience monitoring juvenile fish habitat use. Hansen has been working with for the USFWS IEP juvenile monitoring program for the past three years, and before that as a marine biologist studying marine mammals. Bellmer has been with the USFWS as a supervisory fishery biologist for two years, and with NOAA Fisheries for the seven years prior. He has two publications on the implementation of essential fish habitat. A biographical sketch is not provided for the project lead fishery biologist which makes it impossible to evaluate the overall technical expertise of the project team. The infrastructure within the USFWS and the project oversight by the co-project leaders lend strong support for project success, but the lack of technical expertise regarding otolith application and a sound methodological approach to determine fish residence patterns are areas of concern. This lack of experience**

	in measuring migratory salmonid habitat residence and use of otoliths to track differences in growth is apparent in the time allotted to complete these tasks. While the team has substantial knowledge of habitat quality which is evident by the thorough protocol that is described in detail for selecting their study sites (pages 2–4, and Figure 1); it is evident that their experience using otolith microstructure and morphology is limited (page 5, brief description) to what they have read from the otolith literature (6 citations presented in 6 's). There was no mention in the proposal of how otolith measurements would be made (i.e. image analysis system), or even if the necessary equipment was available for use. The overall success of this project will hinge on the ability of the project team to acquire a lead fishery biologist that has a high degree of expertise in coordinating otolith research and designing studies monitoring fish habitat use.
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget appears to adequately represent the work outlined in the proposal. The only equipment requested to carry out the study which is not already owned by USFWS is the CW tagging equipment and the coded wire tags themselves (n=50000). The tag injectors and tag placement readers will be rented for the duration of the project at 60% of the purchase price to keep equipment costs down.** **The only part of the budget that I disagree with is the fact that there are not enough technicians requested to complete the lab work in a timely fashion. This lack of foresight will inevitably reduce the overall success of**

	the project. I foresee one of two outcomes as a result, either the number of otoliths that get analyzed will be need to be drastically reduced or there will need to be at least one additional technician included in the budget. Reducing the number of otoliths analyzed is not really an option since an equal number of observations are needed from each trial and across the range of different habitat qualities.
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	**Below is a bulleted list of pluses (+) and minuses (−) I used in evaluating my overall review of this proposal.** **+ Primary staff is highly experienced and capable of managing large−scale projects. + Project addresses an interesting question that became apparent from monitoring data + Project team has experience working on juvenile fish habitat projects + May provide a better understanding of the relative importance of Nearshore, shallow habitat − Modifications in the experimental design are necessary to maximize study effectiveness − Authors don't clearly link how their research may lead to management/restoration decisions (flow augmentation, levee removal, restore first order tributary habitats?) − No apparent experience conducting otlolith research, methodology not well defined − Habitat selection will take place during the first year of funding, limiting field trials to 2yrs − Project should focus on all types of freshwater habitats used by wild chinook salmon − Study design should allow measurement of fish residence without terminating fish existence**
Rating	

	fair

Technical Review #2

proposal title: Rearing of chinook salmon fry in the Sacramento River

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals of the project, and the hypotheses to be tested are clear and consistent, and the idea is important. As the proposal states, the more we know about juvenile salmon rearing behavior, the better we can design restoration and water projects to provide quality habitat.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Generating more information about salmon rearing ecology is important. As the proposal states, the more we know about juvenile salmon rearing behavior, the better we can design restoration and water projects to provide quality habitat. However, their analysis of background information may be flawed. They seem to discount the effect of temperature on growth. On page two, they discuss how larger fish are captured at a site later in the season, and provide this as questionable evidence for residency. However, those dates are also later in the season, and weather is**

	warmer. Fish growing at higher temperatures might have faster rates of growth, so be larger when caught at the same site, but not actually be older than those caught in previous weeks. The proposal also equates a high growth rate with fitness. This may or may not be true, depending upon ecological conditions. Larger juveniles require more food. If food is not available, staying small may be a very good move, and lead to higher survivorship. Survivorship, and recruitment, must be somehow examined to tell whether high growth rates are good, and whether rearing under conditions that favor fast growth really leads to high survivorship, if we want to relate environmental conditions to fitness.
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**There are a number of problems with the research methodology, related to their idea that food availability is the determining factor underlying growth rate and fry choice to be resident. Several ecological factors other than food availability can affect growth rate, such as temperature and the need to avoid predators. They state on page two that they do not expect temperature to vary between habitat types. I would expect it to vary, and for temperature to be increased, especially near shore, at the medium and poor sites with less overhanging and emergent vegetation. There will probably be differences in residency and growth at their good, medium, and poor sites, but for several reasons, not just food availability. They could take other data at each of**

	their sites to evaluate other attributes of the habitat. They may not include enough sample sites to make strong inferences. It is too bad that the types of sample sites cannot be distributed randomly in reaches, but that may be the nature of the river system. In the section on recapture, they plan to sample very frequently, to sample until catch rate reaches zero, then to sacrifice all the fish. It seems like it might be better to use a different tagging system, such as injectable paint, that can be detected without sacrifice. That means not having as much age data, but it also means they can find out how long fish stick around, even after capture. Above I suggest having more sampling locations. As a workload trade-off, they could half the number of recapture days, maybe 4th, 8th, 12th, 16th? They could sample more sites, and if they released fish captured in the first 3 sampling events, reduce stress on the fish? Overall, the recapture strategy needs more thought. Another way to test these hypotheses, which would offer greater control, would be to run this as a controlled experiment, using hatchery spawned fry in pens. This would not capture the choice of residency, but would be able to test whether different types of rearing habitat led to different growth rates.
Rating	**fair**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The study could be done as proposed. The author could carry out the data collection as proposed. However, the statistical analysis of such a small sample size would not be very powerful, thus may not fully test the hypotheses.**
Rating	**good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The statistical analysis of such a small sample size would not be very powerful, thus may not fully test the hypotheses.**
Rating	**good**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The P.I. is the only known staff member on the proposal. He has conducted a great deal of research. His familiarity with salmonids is not evident in his body of publications, however, but certainly others within the FWS are very familiar, and the agency should be able to support this kind of research project.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget seems high for the information produced. The way labor costs are presented is a little confusing, and it seems like more days are budgeted for some tasks than may be necessary, especially in Task 1. Altering the study design to require less lab processing could reduce labor costs. Also, on overhead, doesn't the agency overhead usually include administrative and employee support? If not, then what does the 18% go for?**
Rating	**fair**

Overall

Provide a brief explanation of your summary rating.

Comments	**The research questions guiding this proposal are extremely important. However, the research design might not be the best way to answer them, and some of the assumptions underlying the research design may be faulty. On the other hand, the results of the research as designed would be useful, just not quite as useful as they could be. I would like to give it a grade somewhere between good and fair, but I will choose good since I have to choose.**
Rating	**good**

Technical Review #3

proposal title: Rearing of chinook salmon fry in the Sacramento River

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The overall goal of identifying and prioritizing the availability and quality of chinook fry rearing habitats seems important for water management purposes. Understanding basic habitat requirements, when and where such key rearing habitats are located spatially and used temporally, of the wild fry during downstream migration is key to mgmt of such a large system. The authors did a good job of articulating this importance and I found the overall purpose of the study worthy of funding. Their conceptual model in Fig 1 I found useful for framing the underlying purpose of the study.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**I was surprised by tne apparent lack of general knowledge of habitat use by wild chinook fry given the long history of studies on the Sacramento River. Perhaps this is due to the focus in the past on hatchery fish? Though I am not a chinook expert per se, it does appear that there is a lack of general knowledge of wild chinook fry habitat reqmts in large**

	rivers, the exception being some cited work on the Columbia, so this study will add to the base of knowledge for large river chinook in general, and for Sacramento RIver chinook in particular. As stated above, I found the conceptual model of Fig 1 helpful in explaining the basis for the study, and knowledge of temporal and spatial habitat use of these wild fry is critical for mgmt of the species.
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**I had several questions about the efficacy of the sampling design. I liked the idea of habitat classification of the 140−mile stretch of river via GIS, but some ground truthing of the 3 part classification is needed. What are the measurable physical and biological differences (food avail, depth, velocity) between the subjective "good", "med and "poor" habitat classes? Though temporal residency is listed as an important objective,along with growth, of determining habitat quality for chinook fry, it was unclear to me how that was going to be measured. CWTagging of fish in different habitat types was to occur over a 6d period of marking, yet recovery apparently is to occur over some randomly selected 2−14 days of recapture. In short, unclear what the purpose of these varying time intervals was, and unclear how temporaral residency will be determined with this type of sampling scheme? Also, it was unclear what the sampling scheme was. Of the 16 sites sampled each time, where are these to be located along the 140 miles of the study area? If fish are moving downstream during the Dec−Mar sampling period, seems like the entire section would have to be**

	representatively sampled during each sampling period. A figure showing the spatial sampling scheme would have been helpful. Overall, to assess habitat use and growth over such a large area, three measures of habitat quality need to be employed: 1) a measure of relative abundance, 2)a measure of retention rate (relative time spent in a particular habitat type and zone along the river, and 3) growth rate. FOr example, for a poor quality site, it would be hypothesized that relative abundance of fry would be low, there would be high turnover (low retention of marked fish), and those marked fish that were recaptured would show relatively low growth rates. However, I didn't see the study address the first component, and the measure of the 2nd component I did not feel was described in sufficient detail. As noted, I feel the information derived from such a study will be very useful to decision makers, but a bit more clarity on study design is needed to achieve this objective efficiently and effectively.
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The measurement of growth as a function of habitat quality is a key and unique feature of the study that is lacking in most studies of habitat quality. So I commend the authors on including this in their proposal. However, I do wonder if otolith msmt is the most feasible and efficient way of measuring growth. If fry are to individually marked with seq CWT's, then it is unclear to me the advantage of also measuring growth via the much more indirect and labor intensive otolith method. In short, seemed to me that recovery of marked fish in relation to time and size at initial marking would be sufficient to determine relative growth rate? In my experience, otolith growth**

	determination is a very labor intensive and difficult process, especially for the literally thousands of otoliths proposed for analysis in this study. In particular, use of daily growth rings (doubt this is doable with a dissecting scope as proposed) requires a lot of expertise that the authors did not seem to have experience with by my reading of their background. So suggest omitting that part of the proposal as the gain in information, and the quality of the information gathered, is questionable in my view. A subsample of otoliths could be recovered and later analyzed under the auspices of a future study once some intial evaluation of the efficacy of this approach had been completed. I also question if sequential CWtagging of fish is really warranted for all fish captured. Though the data quality from individually tagging 48,000 fry would be high, I wonder if the quality of the data is sufficiently balanced with the downside of having to sacrifice so many wild chinook fry to recover the tags. For fish larger than 50 mm at least, using CWTags as a batch mark (see recent paper on this by Munro et al NAJFM 23:600, 2003) at a site would allow msmt of retention and an indirect measure of growth (mean length of batch marked fish during tagging and during recovery) without sacrifice of the fry. If sites are long ways from each other, then mixing of batches during a particular marking run would likely be minimal.
Rating	**fair**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The authors have proposed a very ambitious project to monitor fry habitat use over a very large section of river (140 miles!). This, in and of itself, is a very nontrivial task. Again, I like their hierarchical approach but believe that a intensive sampling at a**

	few sites, as they've proposed, is likely to have less of a probability of being extrapolatable to the very large (unsampled) area. A sampling design employing more extensive sampling at many sites, in my view would be a more efficient way to sample over such a large area.
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**I agree that the information is critical for mgmt of wild chinook fry and would likely be very helpful for addressing different water mgmt scenarios.**
Rating	**very good**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The Office appears to have a long history of sampling on the river. The background of the PIs specific to this type of project is a bit unclear, but appears they are bringing some fresh perspectives to sampling wild fry habitat which is good. I commend them on tackling such a project over a huge scale, but think that some aspects need some more careful thought and explanation so that results can be applicable to such a large area. In short, I like the overall idea, I like their approach and hypotheses, just think their**

	sampling design needs some more detail. Also, I assume some study is capturing fry when they hit the estuary. Will CWtagged fish from this study be recovered and can the data be incorporated to shore up this proposed study?
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**More of a question than a comment, but the proposed budget is one I would have expected from a soft money organization like a consulting firm--cost of overhead for buildings etc and salary for permanent employees included, rather than an agency where these are included in their annual budget. In short, I understand that agencies must cover operating costs and costs to hire new employees to cover work, I expected that, but was surprised to see other costs that I thought would already be covered under regular support for an federal office (ie., building costs, salary for permanent employees, etc.). My experience is that these are considered inkind support so was surprised to see them as actual costs on this project. Not a criticism but just a surprise to me to see this in the budget calculaion...**
Rating	**not applicable**

Overall

Provide a brief explanation of your summary rating.

Comments	**The overall idea and general approach of the study is interesting and would have imporatant mgmt applications. Sampling over such a large area and**

	capture of so many fish is commendable and needed, but more details of sampling design is warranted, especially gains of fish sacrifice to gains in quality of information.
Rating	**good**